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THE EFFECTS OF EARLY FOOD DEPRIVATION UPON ADULT LEARNING

VICTOR H. DENENBERG AND JAMES C. NAYLOR

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One hypothesis as to why early deprivation affects later behavior is that the early experience modifies the drive state of the organism. Beach and Jaynes have stated that " . . . the effects of early experience might be expected to persist because of the intensity of the motivation under which the experience originally occurred . . . " (1, p. 258), and Hunt (2, 3) has interpreted his hoarding data in terms of the motivating effects of the infantile feeding schedule. The hypothesis suggests that early deprivation leads to a heightened drive level in adulthood when the organism is again placed into a stressful situation.

If Ss deprived in infancy have a higher drive level than Ss not so subjected to early deprivation, when both are placed on a deprivation schedule in adulthood, it follows that equal amounts of food used as reinforcement for correct performance would not be equally reinforcing to both groups. The prediction is that the food would be more reinforcing to the early deprivation group and should lead to better learning. This is the hypothesis tested in the present study.

METHOD

Early Experience. — At 21 days of age, 20 Wistar strain rats were weaned, individually caged, and randomly divided into a control and experimental group of 10 Ss each. The controls were maintained on ad lib Rockland Rat Diet and water until adult testing. The experimentals were kept on a 6 gram per day deprivation schedule, with water always present, for 15 days. After this they were treated in the same manner as the controls. One experimental S died prior to the time of testing.

Apparatus. — This consisted of a four door discrimination box approximately 12 in. x 12 in. x 18 in. Three of the doors were white and one black, the latter being the positive stimulus. Two barriers were present, an opaque one immediately in front of the stimulus doors, and a transparent one several inches in front of the opaque barrier. The stimulus doors were changed after each trial while both barriers were down, after which the opaque panel was raised; 5 sec. later the transparent barrier was raised.

Procedure. — For convenience the 19 Ss were split into two testing groups. One group of 5 experimentals and 5 controls was tested at 130 days, and the other group, consisting of the remaining 4 experimentals and 5 controls, was tested at 170 days. At the beginning of training the Ss were placed on a deprivation schedule of 8 grams a day. Each S was habituated to the apparatus 6 days with the stimulus doors absent. They were then given 10 trials a day using random Gellerman series to the criterion of two successive perfect runs, but with the restriction that all Ss had to be tested for at least 13 days. The noncorrection method was followed. The food pellets used as reinforcement were specially made, so that all were uniform within narrow limits.

RESULTS

Analyses were made of days to criterion and performance over the first 13 days. Preliminary analyses were made to see whether the 130 day and 170 day groups differed from each other. No significant differences were found between the groups and the data were pooled. The experimental *Ss* had a mean of 11.89 days to criterion, while the mean score for the controls was 13.10. Though the means fell in the predicted direction, the difference is insignificant ($t=1.30$; $df=17$). The learning curves for the experimental and control *Ss* over the first 13 days are shown in Fig. 1. Significant differences beyond the .01 level were found for Treatments ($F=10.75$; $df=1, 17$) and Days ($F=58.71$; $df=12, 204$). The Days x Treatments interaction was insignificant ($F<1$).

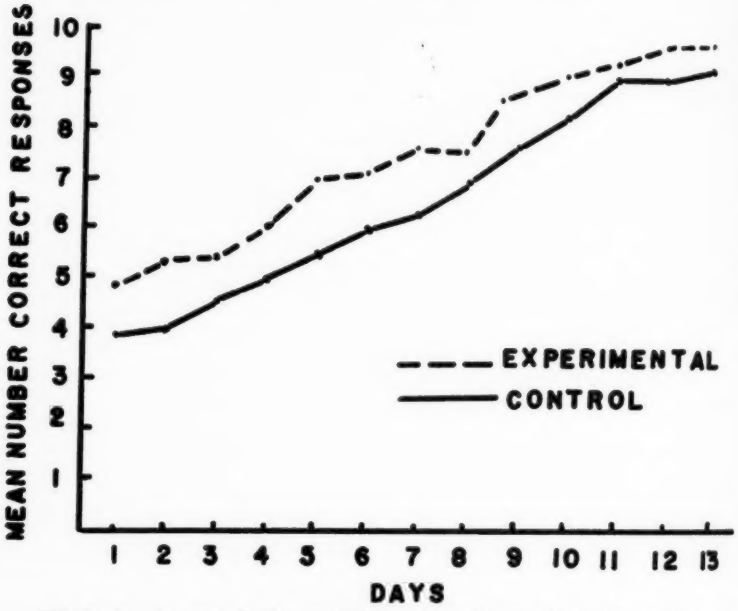

FIG. 1. Learning curves for the experimental and control groups for the first 13 days.

DISCUSSION

The data support the hypothesis that equal amounts of food are not equally reinforcing when *Ss* have received differential food experiences early in life. This, in turn, supports the more basic hypothesis that one effect of early deprivation is to increase the drive state of the organism when subjected to a stressful condition later in life.

The reason for the initial separation of the two curves in Fig. 1 is not known. Though the difference between the means of the experimental and control group on the first day of testing is not significant ($t=1.30$; $df=17$), the overall trend of the curves seems to indicate the general superiority of the experimental group from the onset of testing.

The experimental Ss may have had a higher initial preference for black over white due to secondary associations formed while being maintained on the deprivation diet. These Ss were given their 6 gram ration during the late afternoon or, occasionally, early evening. It may be possible that dusk or darkness acquired secondary reinforcing properties through this process and that this was generalized to the black card when the animal was again placed on a deprivation diet and tested in adulthood.

SUMMARY

The hypothesis under investigation was that equal amounts of food are not equally reinforcing when Ss have received differential food experience early in life. An experimental group of 9 Wistar strain rats was maintained on a six gram deprivation schedule for 15 days following weaning. The control group of 10 Ss was on an ad lib schedule for this interval. Both groups were kept on an ad lib schedule until adulthood, when they were deprived and trained in a black-white discrimination task. It was found that the experimental group learned the discrimination problem significantly better than did the control group, as predicted by the hypothesis.

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The Psychological Record, 1957, 7, 79—85.

PSYCHOMOTOR LEARNING IN DEFECTIVES AS A FUNCTION OF VERBAL PRETRAINING

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The concept of response-produced stimulation has played an increasingly important role in recent years in psychologists' attempts to explain human and animal behavior. Involved in this concept is the notion that internal stimuli are produced by the responses of an organism. Such stimuli are assumed to possess the same characteristics as stimuli which impinge on the organism's receptors from the external environment. That is, it is considered that response-produced cues are capable of eliciting responses and that S-R connections (where S refers to a response-produced stimulus) acquire habit strength in the conventional sense in which S-R psychologists use the latter concept. In the articulate human organism, a substantial proportion of such stimulation is believed to be produced by the overt and covert verbalizations of the individual.

An important implication of this conceptualization for workers engaged in training individuals of either normal or defective intelligence is that the trainer can make profitable use of the stimulation produced by the trainee's own verbalizations in the learning process. In this connection, Miller (12) has written:

> According to stimulus-response theory, learning to respond with highly distinctive names to similar stimulus situations should tend to lessen the generalization of other responses from one of these situations to another since the stimuli produced by responding with the distinctive name will tend to increase the difference in the stimulus patterns of the two situations. Increased differentiation based on this mechanism has been called acquired distinctiveness of cues.

Several studies concerned with this facilitation phenomenon have been reported. A commonly used experimental design has involved a transfer task situation in which Ss are required to respond differentially to certain discriminable stimuli. This task is preceded by a pretraining situation in which one group of Ss (termed here the "relevant" group) is taught names for these same stimuli, while a second group (termed the "irrelevant" group) learns names for a different set of stimuli. Thus, each group of Ss enters the transfer task situation with a comparable amount of "warm-up" experience, but only the relevant Ss have been given names for the stimuli to be discriminated in that situation. According to the acquired distinctiveness of cues formulation, the relevant group would be expected to perform significantly better than the irrelevant group on the transfer task.[1]

The paradigm just described can be criticized in terms of its adequacy for testing the theoretical implications. Should the relevant group exceed the irrelevant group on the transfer task, it might be argued that the superiority is due not to the utilization of verbal cues, but rather to what Kurtz (7) has called

[1]The terms "relevant" and "irrelevant" have been suggested by McAllister (9).

"Observing responses" built up during relevant pretraining. Norcross and Spiker (11) utilized a treatment condition suggested by Kurtz to control for such responses. In addition to a relevant and an irrelevant group, they pretrained a third group of Ss who were exposed to the relevant stimuli, being required to respond "different" when the two relevant stimuli were presented simultaneously and "same" when two reproductions of one or the other relevant stimulus were shown. Norcross and Spiker found the relevant group to be superior to the two control groups, with no significant difference occurring between the latter two groups on the transfer task.

McAllister and J. Cantor (10) question the utility of this "same-different" treatment on the grounds that it fails to provide warm-up experience exactly comparable to that supplied by relevant and irrelevant pretraining. They see no way at present of separating out the effects of observing responses and of verbal cues without altering the type of warm-up experience provided.

The studies reported to date have utilized as Ss normal adults (1, 3, 4, 5, 9, 13) and normal children (2, 6, 11). The present paper reports an experiment in which mental defectives served as Ss. Due to a limited source of Ss, treatments were restricted to use of the relevant and irrelevant pretraining techniques.

METHOD

Subjects.— The Ss were 38 males from the Tennessee Clover Bottom Home, a state residential institution for mental defectives.[2] The age range of the Ss was 19 to 40 yrs.; the IQ range (revised Stanford-Binet, Form L) was 33 to 77. Half the Ss were assigned at random to a "relevant" group, the other half to an "irrelevant" group. Table 1 shows the composition of the two treatment groups.

TABLE 1

CHARACTERISTICS OF THE TREATMENT GROUPS

GROUP	N	AGE (Yr.)		IQ	
		MEAN	SD	MEAN	SD
RELEVANT	19	29.05	5.71	52.63	11.46
IRRELEVANT	19	27.79	7.10	50.10	9.70

Procedure.— In the pretraining situation each S was required to associate the name "hob" with one of a pair of geometric forms and the name "zil" with the other form. Every S was given identical treatment in pretraining, except that the forms labeled "Pair A" in Figure 1 were utilized for Ss in the relevant group, while those labeled "Pair B" were used for the irrelevant group.

During pretraining S was shown the appropriate pair of figures and told the name to be associated with each of the stimuli. The forms were then presented singly to S in a pre-determined, non-alternating sequence. The S's errors were corrected by E and training was continued until S responded correctly to nine out of ten consecutive presentations.

[2]The writers are grateful to Dr. Willard Segerson, Superintendent of Clover Bottom Home, for his cooperation.

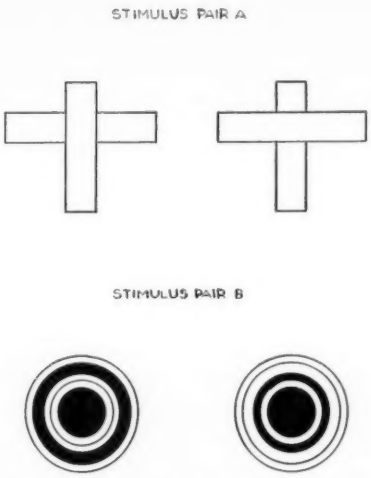

FIG. 1. The geometric forms utilized as stimuli.

Figure 2 shows the transfer task apparatus, as viewed by S.

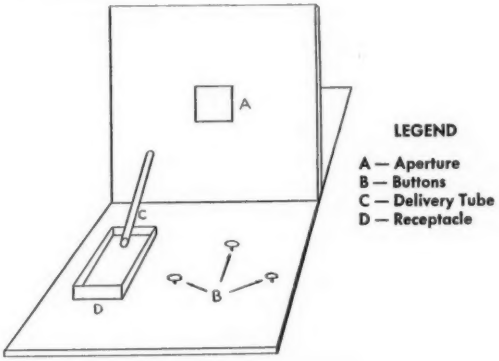

FIG. 2. The S's view of the transfer task apparatus.

The S sat facing the apparatus with his attention focused on the aperture in the vertical screen. A delivery tube inserted in the screen slanted down toward S's side of the apparatus. A receptacle placed under the end of the delivery tube and three buttons were located on a horizontal panel in front of the screen.

Behind the screen was mounted a circular drum, designed to expose one geometric form at a time through the aperture. The drum contained five copies of each of the two forms comprising stimulus Pair A. The ten forms were arranged in a non-alternating sequence on the drum.

The buttons were wired to a solenoid mounted in the delivery tube and to a selector switch located on the back of the vertical screen. When the selector switch was in one position, pressure on the right button activated the solenoid. When the switch was in a second position, pressure on the middle button activated the solenoid. The left button was never allowed to activate the solenoid.

In the transfer task situation *S* was told that two pictures would appear, one at a time, in the aperture and that his task would involve learning to press one button in response to one of the forms and a second button in response to the second form. Before each stimulus presentation, a steel bearing was inserted in the delivery tube. When *S* responded to a given stimulus with the "correct" button, the delivery tube solenoid was activated, allowing the bearing to roll into the receptacle. The *S* was instructed to push the buttons, one at a time, when a picture appeared in the aperture; he was told to continue responding until the correct button was pushed. A pair of socks was shown to S, with the promise made that he would win the socks if he obtained enough of the bearings.

The task for each *S* involved learning to associate the right button with one stimulus, the middle button with the other stimulus, and to eliminate responses to the left button. Every *S* was given 60 presentations of the stimuli — 30 of each of the two Pair A figures. The criterion measure was an error score determined by counting the number of times incorrect buttons were pushed in the 60 trials.

Having been allowed to continue responding on every stimulus presentation until the correct button was pushed, each *S* obtained a total of 60 bearings which he exchanged at the close of the experiment for a pair of socks.

It was predicted that the relevant group would commit significantly fewer errors on the transfer task than would the irrelevant group.

RESULTS AND DISCUSSION

In order for the results on the transfer task to be interpreted meaningfully, it must be shown that the two treatment groups received comparable amounts and kinds of experience in the pretraining situation. Should one of the groups have required more trials than the other to meet the learning criterion in pretraining, for example, it could be argued that the groups differed in the amount of warm-up they derived from the pretraining experience. Subsequent performance differences on the transfer task could then be attributed to this discrepancy in amount of warm-up.

Table 2 shows the results of the pretraining phase of the experiment.

TABLE 2

PRETRAINING RESULTS: TRIALS AND ERRORS TO CRITERION

GROUP	TRIALS		ERRORS	
	MEAN	SD	MEAN	SD
RELEVANT	39.21	19.52	13.84	9.73
IRRELEVANT	32.16	25.25	12.00	13.19

Testing the difference between the trial means yielded a t value of .94 (df=36), while a t of .48 (df=36) was obtained when the difference between the error means was evaluated. Neither of these t values approached statistical significance.

Table 3 shows the results of the transfer task.

TABLE 3

TRANSFER TASK RESULTS: ERRORS COMMITTED

GROUP	MEASURE	
	MEAN	SD
RELEVANT	26.63	17.21
IRRELEVANT	39.10	14.41

It will be seen that the difference between the means is in the predicted direction. The transfer task data were divided into six blocks of ten trials each and Lindquist's (8) Type I analysis was performed. The results of the analysis of variance are shown in Table 4.

TABLE 4

ANALYSIS OF VARIANCE OF THE TRANSFER TASK ERROR DATA

SOURCE	df	MEAN SQUARE	F
BETWEEN Ss	37		
Treatments	1	246.35	5.57**
Error (b)	36	44.26	
WITHIN Ss	190		
Trial blocks	5	140.33	21.96*
Interaction	5	2.79	<1.00
Error (w)	180	6.39	
TOTAL	227		

*P <.001
**P <.025

As indicated in Table 4, the treatments effect was significant at the .025 level. There was no interaction between treatments and trials, reflecting the fact that the relevant group performed better than the irrelevant group during the early learning trials and that this superiority was maintained at about the same level throughout the course of the learning. The performance curves for the two groups are shown in Figure 3.

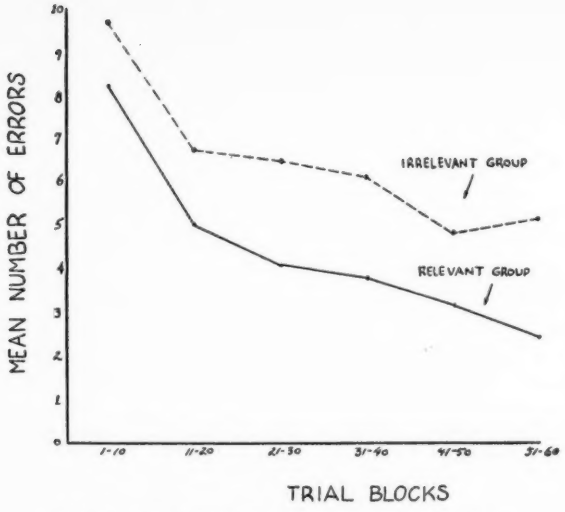

FIG. 3. Mean number of errors made per ten trial block.

The results substantiate the prediction that the group having names for the stimuli in the transfer task would perform better on that task than would the group not supplied with names for these stimuli. How much of the obtained facilitation was due to observing responses and how much to verbal responses learned in pretraining remains a moot question. The study does suggest that, in training the defective to make differential motor responses to stimuli in the environment, such learning can be facilitated by teaching the individual names for the stimuli involved.

SUMMARY

The present study was concerned with the applicability of the acquired distinctiveness of cues formulation to the learning behavior of mental defectives.

Thirty-eight defectives were given a motor task in which they learned to push one of three buttons in response to one visual stimulus and a second of the three buttons in response to a second stimulus. Half the *Ss* — the relevant group — had previously been given a pretraining experience in which they learned names for the stimuli involved in the motor task. A second group of *Ss* — the irrelevant group — was given a comparable pretraining experience, except that they learned names for stimuli unrelated to those of the motor task.

The two groups did not differ significantly in number of trials required to meet the pretraining learning criterion or in number of errors committed in pretraining.

On the motor task the relevant group, i.e., the *Ss* who had been given names for the motor task stimuli, committed significantly fewer errors than did the irrelevant group. There was no interaction between treatments and trials, indicating that the superiority of the relevant group was maintained throughout the motor task performance.

It was concluded that the results were consistent with the prediction made on the basis of the acquired distinctiveness of cues formulation, and that this facilitation effect can be shown to occur in the learning of mental defectives.

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EDWARDS PERSONAL PREFERENCE SCHEDULE INTERCORRELATIONS FOR TWO GROUPS[1]

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PROBLEM

Edwards' *Manual* for the Personal Preference Schedule (PPS) characterizes the intercorrelations for the sixteen variables as " . . . quite low . . . The low (absolute) [author] values . . . indicate that the variables being measured by the PPS are *relatively* (author's italics) independent" (1, p. 12). This study attempts to ascertain the extent to which the intercorrelations for a second college population will be similar to those obtained by Edwards' normative college group.

PROCEDURE

Eighty-two male and forty-eight female undergraduate students at the University of Miami were administered the PPS. Table 1 compares the age data for the local group with that for Edwards' standardization college Ss. It may be seen from this table that the age ranges are somewhat wider for the normative

TABLE 1

AGE RANGE AND MEDIAN AGE IN YEARS FOR EDWARDS' AND LOCAL COLLEGE GROUPS

MEASURE	EDWARDS			LOCAL		
	MALE	FEMALE	TOTAL	MALE	FEMALE	TOTAL
RANGE	15 - 54	15 - 59	15 - 59	19 - 51	18 - 49	18 - 51
MEDIAN	22*	20*	21*	22	20	21

*Estimated from Table 1, Edwards (1, p. 12)

population. The median ages, however, are similar. The raw scores for the combined 130 male and female Ss were intercorrelated for all sixteen PPS variables[2] and compared with the intercorrelations for Edwards' group.

RESULTS

Table 2 presents Edwards' (upper figure) and the local group (lower figure) intercorrelations for the males and females combined.[3]

[1]This study was made possible by a grant from the Penrose Fund of the American Philosophical Society. The author wishes to thank Dr. Raymond E. Hartley, Mr. Jeffrey I. Dallek, and Mr. Don Lichtenstein of the Department of Psychology for their assistance in collecting and organizing the statistical data.

[2]The variables are defined in detail in Edwards (1, p. 5).

[3]Edwards' matrix is taken from his *Manual* (1, p. 12).

TABLE 2

INTERCORRELATIONS FOR PPS VARIABLES FOR MALES AND FEMALES COMBINED
(UPPER FIGURE: EDWARDS' GROUP, LOWER FIGURE: LOCAL GROUP)

VARIABLES	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16
1. Achievement	−.17 −.21	−.05 −.17	.03 .15	.14 .16	−.33 −.12	−.09 .02	−.14 .07	.19 −.05	−.28 −.22	−.30 −.31	−.14 .10	.07 .02	.02 .09	.09 −.05	.10 .13
2. Deference		.26 .41	−.22 −.30	−.30 −.40	.08 .10	.10 .11	−.09 −.10	−.22 −.18	.16 −.14	.05 .06	−.09 −.20	.22 .29	−.28 −.32	−.31 −.29	−.12 −.03
3. Order			−.21 −.17	−.15 −.28	−.16 −.10	−.06 −.13	−.08 −.19	−.16 −.16	.02 .07	−.16 −.04	−.18 −.32	.33 .48	−.16 −.31	−.16 −.22	−.06 .00
4. Exhibition				.09 .06	−.08 −.05	−.22 −.34	−.02 .04	.11 .06	−.18 −.15	−.17 −.19	.12 .13	−.27 −.27	.12 .22	.11 .13	.00 .00
5. Autonomy					−.33 −.34	−.10 .11	−.21 −.10	.07 −.09	−.26 −.21	−.36 −.37	.15 .27	−.13 −.17	.09 .16	.29 .26	.11 −.06
6. Affiliation						−.01 −.23	.09 .25	−.12 −.02	.09 −.09	.46 .34	.06 −.03	−.15 −.16	−.21 −.13	−.33 −.36	−.04 .03
7. Intraception							−.16 −.40	−.12 −.01	−.01 −.08	.07 −.10	−.10 −.09	.03 .13	−.19 −.27	−.20 −.16	.06 .16
8. Succorance								−.22 −.19	.11 .16	.16 .22	−.18 −.06	−.31 −.34	.07 −.10	−.01 .03	−.09 −.07
9. Dominance									−.34 −.30	−.20 −.02	−.11 −.13	−.16 −.18	.04 .15	.21 .13	.02 .09
10. Abasement										.23 .27	−.11 −.24	.07 .09	−.29 −.23	−.25 −.11	−.05 −.15
11. Nurturance											−.12 −.19	−.12 −.05	−.21 −.21	−.33 −.39	.00 .00
12. Change												−.14 −.25	−.07 .14	−.08 −.04	.00 −.07
13. Endurance													−.27 −.31	−.22 −.18	−.06 .02
14. Heterosexuality														.15 .26	.01 .07
15. Aggression															.05 .07

For Edwards' data r must reach .05 to be significant at the 5 per cent level, and .06 to be significant at the 1 per cent level. For the local group data an r of .17 is significant at the 5 per cent level and an r of .23 at the 1 per cent level. Inspection of Table 2 discloses a marked variance in the extent of significance among the intercorrelations. Table 3 shows the distribution for three levels of significance of both sets of intercorrelations. It may be seen from Table 3 that the test responses of the local group of male and female college students yield less than one-half the significant (at the 1 and 5 per cent levels) intercorrelations reported by Edwards. A complementary analysis of the nonsignificant coefficients shows five times as many correlations in this category for the local group as compared with the standardization population.

TABLE 3

DISTRIBUTION BY SIGNIFICANCE LEVEL OF EDWARDS' AND LOCAL GROUP INTERCORRELATIONS

GROUP	SIGNIFICANCE LEVEL			TOTAL
	.01	.05	NON-SIG-NIFICANT	
EDWARDS'	99	7	14	120
LOCAL	32	18	70	120

Returning to the intercorrelation matrix in Table 2, it will be noted that the directionality (i.e., plus or minus correlation sign) of the 120 sets of coefficients is similar in 101 cells — 84 per cent agreement. The remaining nineteen pairs of correlations do have sign differences within some individual cells. However, since none of these is significant at or below the 5 per cent level, they may be considered to be uni-directional sets along with the other 101 pairs of coefficients.

DISCUSSION

The two college groups responded somewhat similarly to the verbal symbolizations of the needs that the variables represent. Differences in the degree of variable-interrelatedness are reflected in the significance levels and absolute sizes of the obtained correlation coefficients.

Edwards' larger sampling group, 1509 college men and women, was a far more heterogeneous group than the local student group of 130 men and women from a private university. Since the number and heterogeneity of Ss will affect the size of a correlation, it is not surprising that Edwards' intercorrelations yielded a higher percentage of significant coefficients than the more homogeneous local group. Actually, however, the difference in the percentages of significant and nonsignificant correlations is not as critical as it appears to be in Table 3.

For the 1 per cent level, an Edwards' intercorrelation must reach .06 as contrasted with the local group's *r* of .23 for the same confidence level. An inspection of the pairs of correlations in the matrix of Table 2 readily discloses 94 local group coefficients which exceed in absolute size the Edwards .06 figure. Furthermore, in all instances of high coefficients, the two portions of the individual cells are fairly close to each other quantitatively.

With regard to the absolute sizes of the significant correlations, the local correlations yield four substantial coefficients (.48, .41, —.40 and —.40) as compared to only one (.46) for the normative group. Using Guilford's (2, p. 165) criteria for the strength of the relationship between correlations, it may be stated that the present study generally supports Edwards' conclusion that the PPS variables are *relatively* independent.

Specific attention, however, must be directed to the several pairs of correlated variables whose absolute sizes do reveal a substantial interdependence rather than independence. These are: Affiliation-Nurturance, *r* of .46 (Edwards' data); Order-Endurance, *r* of .48; Order-Deference, *r* of .41; Deference-Autonomy and Succorance-Intraception, both with an *r* of —.40. First, the three substantially, significantly, and positively related variables will be discussed in the light of Edwards' definitions (1, p. 5):

1. *Affiliation-Nurturance*, as a covarying pair, share the common element of social identification, i.e., a need to make warm interpersonal ties with "giving" as an essential ingredient.

2. *Order-Endurance* are directly related in terms of an underlying personality trait best described as perseverance or "stick-to-itiveness."

3. *Order-Deference* does not reduce readily to a common personality affinity on the basis of Edwards' descriptions of these two variables. However, it is possible that the element of an "inner-" or self-directed orientation toward people and events is the essential need-dimension.

Second, the two substantially, significantly, and negatively related variables:

4. *Deference-Autonomy* are obviously at opposite ends of the personality dimension best described as the dependency-independency continuum. Thus, PPS items relevant to these two manifest needs tend to be mutually exclusive as overt expressions of the dependency-independency factor of personality.

5. *Succorance-Intraception* are related in an indirect manner through a shared attribute of a similarity in attitudes toward the self by the individual himself and by others. In other words, both subsume a "me-oriented" mode of dealing with others and of being dealt with by others.

In view of the predominant absence of substantial relationships among most of the PPS variables, as reflected in the fairly small absolute sizes of the intercorrelations, Edwards' claim for the *relative* independence of the sixteen variables is acceptable. However, exception must be made to this statement with specific regard to the *relative* independence of the five pairs of variables discussed immediately above. This does not mean that the sixteen variables do not tap somewhat discrete personal needs. It implies, rather, that items relevant to each of these eight (Order and Deference appear in two pairs) variables are, in a direct or inverse manner, related substantially to each other through a common affinity along a personality continuum for each of the five pairs.

SUMMARY AND CONCLUSIONS

The raw scores on the Edwards Personal Preference Schedule for 130 local college males and females were intercorrelated. These were compared with the correlations obtained by Edwards for his 1509 college students. Both groups were fairly similar with regard to median age and age range.

In general, the intercorrelations for both total groups were similar. Differences that were obtained could be attributed, in the main, to the size and heterogeneity of the two college populations. The directionality (signs) of the coefficients did not differ significantly. While many of the correlations for both groups were significant at the 1 and 5 per cent levels, the absolute sizes of the majority of the coefficients did not contraindicate the correctness of Edwards' contention that the sixteen PPS variables are *relatively* independent. An exception to this general positive statement is a question of interdependence that may be raised in connection with five pairs of variables whose correlations do suggest substantial communalities. These were discussed in the light of a shared affinity for each pair individually, with no implication that more than a specific common continuum is involved.

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PERSPECTIVES IN PSYCHOLOGY

III. SOME COMMENTS ON THE DEFINITION OF EMOTION

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Two problems can be distinguished in regard to a definition of emotion. The first of these is the problem of developing criteria for distinguishing between emotion and non-emotion. The second, which should logically wait upon the solution of the first, is the problem of distinguishing between different types or qualities of emotion. It is a matter of fact, which easily can be verified by reference to the current literature, that proposed solutions to the second problem have flourished in spite of general discontent among psychologists with criteria suggested for handling the first. In this paper we will restrict ourselves to the problem of distinguishing emotion from non-emotion.

Historically, the search for criteria has centered upon the activities of the organism. In the pre-scientific psychology this orientation achieved expression in the concern with subjective "feelings." With the advent of behaviorism attention has swung to overt responses and the more covert physiological activities. Physiological responses have been considered as (1) constituting the emotion, and (2) as being merely indices of the emotion, itself not directly measureable. It would be foolish to claim that all avenues of research suggested by these general approaches to the problem have been exhausted; nevertheless, the fact remains that after years of careful investigation psychologists are little if any closer to a solution of the problem.

Skinner (3), in a recent attempt to find a new approach to the problem, suggests that the scientist has been in a disadvantageous position due to trying to work out a definition using such variables as those mentioned above. According to Skinner, the scientist has not done as well as the layman, who " . . . identifies and classifies emotions not only with ease but with considerable consistency." Furthermore, suggests Skinner, "Some means of identification available to the layman appears to have been overlooked."

Ignoring the possibility that the layman's facility in identifying emotion may be more apparent than real, let us follow where this idea leads Skinner. He believes that the layman looks at emotion as a " . . . predisposition to act in certain ways." Increased probability of occurrence of one type of response is given one name, of another type, another name.

More pertinent to our problem of distinguishing emotion from non-emotion, Skinner defines an emotion in general terms as " . . . a particular state of strength or weakness in one or more responses induced by any one of a class of operations." The most immediate criticism of this definition is that, while it may define emotion, it may also define almost anything else in psychology, for example, learning, perception, neurosis, etc. Still, the notion of having overlooked something is intriguing and we will return to it later.

Another new approach has been suggested by Moore (2), who favors " . . . the application of general psychological theory to experimental investigations of typical and readily recognizable examples of emotion." Moore

suggests that by a thorough investigation of these readily recognizable emotional responses we can garner specific information which can be used to distinguish the general category of emotional conduct from other types of behavior. Moore's example of emotion is the "conditioned emotional response" (CER), which is considered to be a laboratory reproduction of the crouching or freezing behavior found in some sub-human species. He reports investigations in which are established relations between the CER and such variables as age, reinforcement schedules, electroshock and drugs.

While not questioning the general value of such investigations, it does seem important to point out that they will not generate a definition of the phenomenon under study. Although we may test, expand or reject a definition on such a basis, still we do not begin to do experimentation until we have first arrived at some basic definition or definitions. That this case is not an exception seems to be pretty well indicated by the fact that Moore is ready to accept the CER as an example of emotional behavior. Obviously, this would be impossible unless emotional behavior had been at least tentatively defined to Moore's satisfaction. This definition is not given in his paper.

Let us return to Skinner's assertion that the scientist must be overlooking something in his attempts to define emotion. It is the thesis of this paper that two "somethings" have been overlooked. The first of these is that, in spite of the lack of success of the method in other areas, the psychologist has attempted to define the concept of emotion solely in terms of activities of the organism, *ignoring other aspects of the situation.* This possibly represents an historical accident, since the early attempts at an objective definition were aimed at finding a substitute for "feeling" or "awareness," which were themselves presumed to be activities of the organism. Skinner's definition represents a notable shift away from this tendency.

To put the point positively, psychologists have generally overlooked the fact that useful psychological concepts have been those based upon the *relationship* between stimuli and responses, and not on either of these by itself. As in the case of learning, the view of emotion as an asymmetrical event has led us into trouble.

The other factor which has been overlooked concerns the possible role of physiological responses in emotion. It has been held by someone at some time that emotion is the awareness of these responses, that they are indices of emotion, or that the responses are the emotion. None of these positions seems to have worked out satisfactorily, so that while there is general agreement that physiological response is important in emotion, there is lack of agreement or understanding of the role that it plays.

The only definition of emotion known to the author which adequately handles these two problems is the one offered by Kantor (1) in 1926. Kantor's concept of emotion as a "no response" event includes for physiological responses the role of substitute behavior in the more violent events. According to Kantor, we have, here, situations in which " . . . the person may be found lacking a response system in a situation in which the greatest immediate need for definite action seems to dissociate him so completely that he is left with only his simplest elementary behavior . . . "

Except for Skinner's definition, the one offered by Kantor, namely, "a definite type of failure to perform an expected form of adjustment or adaptation upon the basis of surrounding conditions and the individual's reactional biog-

raphy or previous behavior history," is the only one known to the author which is event rather than response based. Wickens and Meyer (4) approach this sort of a definition in regard to a particular emotion which they call "fear," but do not do so in regard to their general definition of emotion.

That Kantor's definition is of a particular type of event is clear, but there might be a challenge as to whether this event could adequately be described as "symmetrical," when the dominant characteristic is the lack of an expected response. This challenge is met by the fact that what is observed here is not a complete absence of response but only the absence of a particular expected response. Further, though the absence of the expected response is stressed as the prime criterion for distinguishing emotion from non-emotion, this facet is no more important than the others in the actual event.

With this definition as a basis for further inquiry, we should not only regain whatever ground we may have lost to the layman in dealing with emotion but also, place ourselves in the advantageous position which the use of scientific method should have given us long ago.

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REDUCTIONISM REVISITED: A BOOK REVIEW

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The psychology of learning. By B. R. Bugelski. New York: Henry Holt, 1956. Pp. xiii+523. $5.75.

As every graduate student knows, Hull's first four postulates (1943) concern afferent and efferent impulses, their interaction, and the effect of reinforcement upon them. As every graduate student further knows, Hull never *really* meant this, since he never did anything to coordinate his conceptual physiology to experimental procedures. Now along comes Bugelski with the startling suggestion that Hull *did* mean this; and that Hull's theory when incorporated with recent developments in neuro-physiology—as explained for psychologists by Hebb—provides the basis for a superior new theory of learning. (The reader should have no difficulty concocting a name for this Hebb-Hull combination.) Bugelski simply argues for the merit of a physiologically-based theory of learning; he does not go on to develop it in detail, nor is this the purpose of his text. Rather, Bugelski states that his text is designed to (a) "clarify the major issues in the controversial fields of learning and motivation" (p. vii), (b) "provide a reasonably comprehensible introduction to the problems over which learning psychologists worry" (p. viii), (c) "make some sense out of the psychology of learning and to indicate the limitations as well as the virtues of learning theory" (p. ix), and (d) integrate the views of Hebb and Hull "to the extent that integration appears possible" (p. viii). No attempt is made (a) to offer a new theory, (b) to include reference to all schools of thought (Lewin, for example, is deliberately left out), (c) to cover the entire area of behavior. This is an extremely ambitious program and one which entails an extensive familiarity with both experimental and theoretical literature, a good deal of critical thinking, and a fair amount of experimental work. In this reviewer's opinion, Bugelski, in attempting to do so many things at once, has succeeded in doing none of them very well, but his attempts merit serious consideration.

Clarification of the Major Issues.

The chapters of the book fall into four categories, roughly paralleling Bugelski's four purposes. The most numerous class are the chapters devoted to major issues: how many kinds of learning are there, continuity vs. discontinuity—the whole hoary host. The first chapter begins with a consideration of current definitions of learning. Bugelski rejects all of them (except Spence's) as vague, useless, or both. Not until the end of Chapter 5 does Bugelski introduce his own definition: "learning is the process of the formation of relatively permanent neural circuits through the simultaneous activity of the elements of the circuits-to-be; such activity is of the nature of change in cell structures through growth in such a manner as to facilitate the arousal of the entire circuit when a component element is aroused or activated" (p. 120). In view of Bugelski's criteria for the rejection of other definitions, how can he offer this one as superior? He states that this definition is "presumably testable eventually," but since when do we test definitions? Are not definitions the preliminary rules for applying terms and procedures which enable us to test theorems and hypotheses? Moreover, in

his concern over a definition of learning, Bugelski fails to explain the terms and principles of theory construction which he uses freely. Nor does he offer any summary of experimental procedure and the reasons for preferring it. Both seem to be serious oversights in an introductory text (frequently, one gets the feeling Bugelski wrote the book not for students, as he asserts, but for his fellow psychologists).

Specific experimental procedures are introduced in the second chapter. Bugelski outlines 12 paradigm experiments (he makes no distinction between demonstration and experiment), each of which is followed by a number of questions designed to stimulate student thinking, e.g., was the response voluntary or involuntary? does the rat know what he is doing? does he press the bar because he wants to? The chapter ends with 12 questions, reminiscent of Hilgard's six criterial questions, which are to be taken up in the rest of the book.

In the third chapter, ostensibly dealing with the capacity of the learner, Bugelski launches one of his many polemics. This one is directed against (a) anti-reductionist theories of learning, which he states are uniformly directed against the outdated physiological theorizing of the '20's; (b) the neglect of Tolman's H A T E variables. In Bugelski's opinion, heredity, age, previous training, and physiological conditions constitute the major variables to be investigated; in neglecting them, learning theorists put the cart of environmental variables before the horse. I question Bugelski's distinction of what is cart and what is horse. A more fruitful breakdown would be (a) variables which can be manipulated and investigated directly, and (b) those which cannot. The first class of variables can provide the basis for a precise (hopefully, a quantitative) theory of learning, while the second class could be introduced as parameters for more detailed description and prediction. Bugelski's implication that the functional relationships themselves are determined by heredity, age, etc., is novel and worth investigating if possible, but I would prefer to see it presented as an assumption rather than an assertion.

In treating the question of how many kinds of learning exist (Chap. 4), Bugelski correctly notes that in practice two bases for classification are employed: experimental paradigms, and explanatory principles; and that much confusion arises from the failure to distinguish them. After so promising a start, he bogs down and finally emerges with the seemingly inconsistent conclusion that: (a) one principle will account for learning under a variety of paradigms, (b) the principles which account for learning in mature organisms (i.e., the neural processes involved) differ from those which apply in infancy. It is not clear whether this assumed difference in processes necessitates a revision only in the associated parameter values or in the nature of the relationships themselves.

The critical consideration of major issues in the area of learning continues in the chapter on learning and performance (Ch. 8). Here Bugelski reviews the different response measures which have been employed and the few studies of their intercorrelations. He concludes that no response measure is very satisfactory, but that number of correct responses is probably the best (for what purpose?). He next considers learning curves and rejects not only the old quest for "the learning curve" but also the meaningfulness of any learning curves.

The preceding chapter recapitulation is intended to illustrate Bugelski's version of "major issues." In general, many of his "major issues" have an old-fashioned air about them, and the discussion of them often impresses the

reader as the beating of dead or dying horses (continuity vs. discontinuity), or of horses which never really lived (hypothetical construct vs. intervening variable). As to clarification of the issues, many of Bugelski's points are well taken (e.g., his preliminary treatment of the number of kinds of learning). At other times, however, clarification seems to consist either in talking away the existence of an issue or in posing another (early vs. late learning). It would seem preferable either to have left out the pointless issues altogether or to treat them much more briefly.

Specific Theories.

Details of specific theories of learning are outlined primarily in three chapters (Ch. 5, 9, 10). Chapter 5 introduces the theoretical units of Hull, Guthrie, Skinner, Tolman, and Hebb without specifying the experimental procedures—if any—to which they are related. Naturally, Bugelski argues for the superiority of Hebb's cell assemblies and phase sequences as basic theoretical units in contrast to such vague or useless units as movements, etc. What makes cell assemblies and phase sequences less vague and more useful is never elaborated (for obvious reasons). Chapter 10 is devoted to the principle of reinforcement and its history; chapter 9 deals with motivation. The motivation chapter is probably the least organized and well documented of the three; a lengthy recapitulation of the Nebraska Symposium does little to improve it.

Data at Last.

Although "The Psychology of Learning" is intended to be an introductory text, only three chapters are primarily devoted to organization of experimental findings (Ch. 6, 11, 12), and these appear relatively late in the text. For the most part, they are straightforward chapters. This reviewer, personally, would prefer more description of procedure and discussion of methodological questions of the type that is provided for retroaction and transfer designs. For the most part, Bugelski includes only a brief summary of procedures and conclusions. Occasionally, the summary of experimental procedure is incomplete, as in the treatment of Pavlov's experimental neurosis study in which Bugelski neglects to point out that S^D was paired with food and S^Δ with shock. Unless it is made clear that punishment was used (inconsistent reward and punishment, from the dog's point of view) the student may get the impression that difficult discrimination is the crucial factor.

The chapter on extinction is a doubtful and disappointing inclusion in the list of data chapters. For one thing, it provides a relatively sketchy treatment of extinction, and it appears very late in the course of the book, although mention of extinction occurs frequently in the earlier chapters, as it must. Furthermore, a good deal of space is devoted to beating upon the concept of reflex reserve which Skinner himself rejected some time ago. On the other hand, very sympathetic treatment is given to Eccles' neural model of acquisition and extinction in Pavlovian conditioning, although Bugelski himself confesses that it does not account well for existing data. Finally, the topic of partial reinforcement is here introduced for the first time, given brief treatment, and more or less dismissed. The possibly peripheral topics of habituation and frustration (in the Amsel sense of non-reinforcement following continuous reinforcement) are not even mentioned. As to theoretical treatment of extinction, Bugelski comes out for an interference explanation and points out that since the same principle best accounts for forgetting, probably extinction and forgetting are very similar processes.

The Limitations and Virtues of Learning Theory.

The last two chapters are devoted to applications of learning principles to personality disorders and to education. In both cases Bugelski feels that learning theory in its current state has little to offer. A possible exception lies in the field of education, where the one concrete suggestion which Bugelski derives from all past work is that it is important to maintain an optimal degree of anxiety! He pleads—and I think rightly—for less sweeping extrapolation from the results of a few laboratory studies, and for a more critical appreciation of the additional variables involved in "applied" problems. Unfortunately, his pleading occasionally takes the form of preaching. The beginning student may come away from these chapters with the feeling that the areas of learning and learning theory are so muddled, controversy-ridden, and irrelevant, that it is useless to waste so much time on them, rather than be motivated to investigate the host of unexplored problems which Bugelski points up.

The Book as a Whole.

Bugelski's new book is chock-full of information, speculation and suggestions, all of which range the quality continuum from one extreme to the other. In comparison with existing texts it fares badly: Deese provides a better organized summary of learning data; Hilgard gives a much better summary of contemporary theories; and Estes, *et al* provide a more telling lesson in the critical evaluation of learning theories. As an introductory learning text, it presents real problems. The beginning student confronted with so many theories and controversies, so much critical (and not-so-critical) evaluation of them, and so little organized background of experimental evidence can at best come away with confusion—at worst with serious misconceptions.

Despite all these objections, I think the book has a great deal of value for the more sophisticated reader. Bugelski is a thoughtful and eloquent spokesman for at least two unpopular views: reductionism and complicationism. With respect to the former, Bugelski seems to me to be naive in suggesting that we push our ignorance back into a conceptual nervous system on the hope that a complete knowledge of its physiology will ultimately solve all the problems of learning. Physiological theorizing may have come a long way in the past 30 years, but I doubt that it will ever come *that* far. Besides, learning theory hasn't been exactly stationary during that period. With respect to experimental work, Bugelski summarizes a number of the ingenious and important studies of Hebb and his followers. These studies do not deserve the neglect they have received from most learning theorists. As for the complicationism, Bugelski pleads for (a) *observation of behavior in context,* and (b) investigation of the effects of a vast number of neglected variables. If either plea makes some of us look beyond our simplified explanations of circumscribed experimental areas, he will have accomplished a lot—although he will not be the first to have tried.